No.	B.D.	R.A. 1880 h m	Decl. ° ′	P. °	D. ″	Mags.	Nights.	Date.
215	+34·4635	10·2	34 18	141·7	8·8	8·8 12·5	1	04·69
216	+35·4850	32·3	36 4	...	4 ±	11 13	1	04·78 BC
				38·0	44·3	A = 8·3	1	04·78 AB
217	+36·4925	40·0	36 17	...	3 ±	10 11	1	04·78 BC
					70 ±	A = 8·7	1	04·78 AB
...	+35·4917	50·1	35 43	243·0	49·6	5·0 8·5	2	04·77
218	...	51·9	64 9	330·5	2·8	11·0 12·0	1	02·73 BC
				296·4	19·1	A = 10	1	02·73 AB (h 1833)
219	+35·5001	23 12·8	35 42	309·8	6·1	9·8 10·2	3	04·96
220	+61·2430	15·6	61 45	...	4·0	11·5 12·5	1	04·02 BC
					30 ±	A = 8·0	1	04·02 AB
221	+35·5153	54·7	36 7	233·9	14·8	8·1 8·8	3	04·87

Notes.

163. Measures discordant.
165. Discordant angles.
168. Discordant angles.
174. 42″ N. 10 sec p B.D. + 36°·1528.
175. h 5284 is south.
182. According to the list of proper motions in the Harvard section of the Catalogue of the Astron. Gesell., this star has a P.M. in Decl of +0″·103. If B was stationary the distance between the stars would have been 0″·9 at the time of the Harvard observation.
184. Discordant angles.
197. Discordant angles.
202. October 8, A*a* too faint to measure, another still further in the same direction. November 12, glimpsed A*a* and thought it the first of three in a line. November 14, A*a* seen—not sure that there is not a nearer and still fainter one.
204. Discordant distances.
205. Discordant angles.
207. Faint and unsteady poor measures.
212. No particulars, simple entered as double.
213. The fainter star of a wide pair.
219. Discordant angles.

Additional Note.—Since the above paper was presented Professor Hussey's ninth catalogue of new double stars has been received, and No. 195 was found to be identical with Hussey 946 :—

195. 8·0 and 10·0. 240°·8 − 5″·25. 1904·47. Hussey. 2.

On the Determination of Proper Motions without Reference to Meridian Places. By Arthur R. Hinks, M.A.

The purpose of this note is to suggest that the application of photography to the determination of the proper motion of stars leads naturally to an inversion of our ideas in respect of the origin to which these proper motions should be referred. The

suggestion is briefly this, that instead of working backwards from what we may for the moment call the foreground of brighter, and on the average nearer and faster-moving stars which are comprehended in the fundamental catalogues, we may, by the application of photography, work forwards from what we will call for the time being the background of most distant, almost stationary, and on the average fainter stars.

The determination of proper motions from meridian observations involves several difficulties of a systematic kind which may be briefly enumerated thus.

1. The systematic corrections from one catalogue to another, even when both are referred to the same fundamental system, and the systematic reductions from one fundamental system to another, are well-known troubles which embarrass every investigator of proper motion, and need no further comment.

2. Personal equation depending on magnitude, which seems to affect with the same sign, though to different degrees, all visual determinations of right ascension, becomes troublesome immediately one attempts to determine the proper motions of stars fainter than the 9th magnitude, near the limit of visual observation; and it introduces into the R.A.'s of all faint stars determined by photography a systematic error depending upon the mean magnitude of the stars adopted as standard. All attempts to determine the value of this visual magnitude equation experimentally, or to eliminate it by process of observation, seem to be unsuccessful. Take as an example the case of the *Repère* stars for the reduction of the *Eros* observations. They were observed at a great number of places. They have been discussed very thoroughly by Professor Tucker, and formed into two normal systems (Paris *Eros* Circular, No. 11). On the strength of determinations of the magnitude equation in the places from Lick and Königsberg, the first by the method of screens, the second by the clockwork transit micrometer, Professor Tucker concludes that his first normal list is free from magnitude equation (*L. O. Bulletin*, No. 72). Yet I find on comparing his list with the photographic places of the stars from six observatories (Bordeaux, Catania, Northfield, Paris, San Fernando, and Toulouse), which have no appreciable photographic magnitude equation relative to one another, that between Tucker's List I. and the mean of these six observatories there is a magnitude equation of about $0^s.02$ per magnitude. I conclude that it is by no means certain that Tucker's System I. is really free from magnitude equation, and that visual and photographic determinations of R.A. cannot at present be brought into accord in this particular.

3. Still more serious is the probability that the constant of precession, necessarily determined from a limited selection of the brighter stars, will be found not applicable to the stars in general, or to other selections. It has been shown by Professor

Kapteyn in his address delivered at the St. Louis Congress,* and also by Messrs. Dyson and Thackeray in their discussion of the proper motion of Groombridge stars (*Monthly Notices*, 1905 March), that the distribution of proper motions is so far from being random that existing determinations of the solar apex are more or less invalidated. But determinations of the solar apex and the precession constant are essentially entangled; if bright stars give a different position of the apex from faint ones, it is more than probable that they will give different values of the precession constant also. At any rate, a first criterion for the choice of stars from which to determine the precession constant is that these motions shall not be systematic over considerable areas of the sky, and it is now certain that the stars which have been actually used do not satisfy that criterion. We can hardly avoid the conclusion that our knowledge of the precession constant is not sure enough to allow us to pass with confidence from the bright stars down the whole range of magnitudes which are within the scope of the photographic method.

Can we then say that our present system of meridian places of bright stars is a sure foundation on which to base the proper motions of faint stars determined by photography? Evidently not; the persistence of magnitude equation in spite of all efforts and the want of homogeneity in the system of the meridian stars forbid it. The former difficulty may be overcome; the latter must prevail so long as star-places are referred to shifting planes of reference whose motion might be adjusted to one homogeneous system of stars observable with meridian circles, but becomes indeterminate if not undefinable when more than one system is involved in the limited number of available stars.

So far as I am aware, the suggestion has not been made hitherto that by the use of photographic methods one may dispense altogether with any reference (except a quite subsidiary one) to these shifting planes, and determine the proper motions of the stars independently of the precession constant. The proposition is evidently true if one admits the possibility of picking out to serve as a background a set of stars so distant that their peculiar proper motions are very small and their parallactic motion infinitesimal. Professor Newcomb gives reasons for believing that the stars extend in every direction beyond a sphere whose radius corresponds to a parallax of 0″·001. The parallactic motion of stars on that sphere would not exceed 0″·4 per century (using Campbell's determination of the solar motion). And since the sphere of *lucid* stars probably extends to half that distance, we might hope to get a background not unreasonably faint.

Now it appears to me that the selection of stars for the back-

* This address has not yet been published. The result that there are two distinct streams of stars has been quoted by Professor Turner (*Observatory*, 1905 February, p. 118). By the great kindness of Professor Kapteyn I have had the privilege of reading a portion of the address in manuscript.

ground becomes a relatively simple matter provided that star-photographs are treated in the simplest possible way. Imagine that we possess for a given epoch a set of photographs on the same centre : exposures, let us say, with an astrographic telescope, of one minute, giving measurable images of stars between 6^m and 10^m; of three minutes for stars between 8^m and 11^m; perhaps of thirty minutes for stars between 10^m and 13^m. Exposures beyond thirty minutes will not be good for measurement over a large field owing to refractional distortion. So let us suppose the series continued with an instrument like the Bruce telescope down to 15^m; and for a small central field with a large reflector down even to 17^m. If rectangular coordinates are measured on all these plates they can be reduced in the simplest fashion one to another, so that in the end we have a system of rectangular coordinates homogeneous (except for instrumental causes) for all the stars in the field—a system, be it noted, whose orientation and scale-value need not be known to any refinement, while it may even involve refraction and aberration, provided that the corrections for all these errors, were they to be applied, would be expressible as linear functions of the coordinates.

Imagine, further, a similar system of rectangular coordinates of the same stars on the same centre (with respect to the stars) made after a lapse of years; and suppose one system reduced to the other *en bloc* by the ordinary linear reduction. The residuals for each star would represent its proper motion relative to the mean of the rest. All stars with sensible proper motions might then be excluded, and a new reduction made with the remainder. The proper motions of the faster-moving stars with respect to this residuum would not indeed be absolute, since they would not include the average parallactic motion of the residuum during the interval between the two epochs. But they would more nearly represent the absolute proper motions than any which could be derived by reducing the measures to R.A. and declination through the introduction of "standard" stars. It is unnecessary to follow out in words the way in which this procedure would work. But it is evident that as the process was repeated and extended there would be a straightforward sifting out of foreground stars; a continual approximation to a motionless background; and an easy way of forming an estimate from time to time of the amount of parallactic motion still in the background by classifying residuals according to magnitude.

The process would be carried on in regions all over the sky, and it would be necessary to select some system of axes to which the motions might be referred. Here, and here only, should we require to know the coordinates of a few stars determined by meridian instruments. But having neglected the existence of the equator and the vernal equinox up to this point, we must not fall from old habit into the mistake of referring the proper motions now to these discarded origins. It would be just as simple and

clearly more advantageous to refer everything to an assumed galactic plane.

We may anticipate the objection that the method is illusory, since it may be true that there is no motionless background; that there may be no observable stars so far off that their parallactic motion is insensible; and that in any case there can be no certainty about a method that does not tie the various regions together and prove that they are relatively motionless. The objection does not appear to me to be sound, since we determine by the photographic method the proper motions of all the meridian stars which are not too bright, and their places with respect to the rest; so that we have at our disposal the whole results of meridian observation to establish the relative fixity of our different patches of background. Yet this harking back to meridian places is not by any means equivalent to an ultimate return to the method which we proposed to abandon. It is essentially different from it in three respects: we use the meridian observations only to give us the absolute distances apart of stars upon the sphere, which can be obtained practically free from any precessional effects; the proper motions of these stars also, the stars observed on the meridian, are determined, not from the meridian observations, but by the photographic method; and the whole determination of proper motion is carried as far as the final step without any reference whatever to the meridian, so that it cannot get entangled with any of the difficulties that beset meridian observation.

A word may be said as to the choice of regions for the application of this proposed method of taking samples of proper motions. It would be a mistake to start *de novo* with a scheme of centres arranged with perfect symmetry after a cut-and-dried plan, because in so doing we should inevitably miss all the things that are of particular interest. On the contrary, one might let the regions to a great extent select themselves. Some of the most interesting regions of the sky were photographed twenty years ago; we should gain twenty years if those photographs could be measured, and an attempt must be made to do so. And there are a dozen other reasons for selecting particular fields—the presence of a group of Wolf-Rayet stars, or of stellar gaseous nebulæ, or of a star cluster which has already been surveyed. The region round a variable star whose photometric magnitudes are well determined would be interesting; so would a nebula like *Messier* 33, whose condensations are so nearly stellar that some of them might be measured; and one or two binaries whose real orbits may be large enough to measure; and some of the regions containing the fainter helium stars; and the fields of those Novæ which survive as faint stars after passing through the planetary nebular stage. When all these obviously interesting things had been provided for, it would probably be found that their distribution was not as symmetrical as might be desired about the galactic plane; but the gaps could be filled in on

Astrographic Catalogue centres. The scheme is in fact potentially in operation, for the astrographic plates have many of them been measured and published in rectangular coordinates, and the age of some of them is already considerable; they provide thoroughly for stars down to the 11th magnitude. The most urgent matter is to discover how good reflector photographs may be for carrying the measures down to the 15th or 16th magnitude. Perhaps I may add that we hope to be able to borrow some plates, and try this at Cambridge in the not distant future.

Cambridge Observatory:
1905 *May* 11.